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Keller and Schoenfeld's *Principles of Psychology*

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It is certainly difficult, and perhaps unnecessary for present purposes, to distinguish between the influence of Keller and Schoenfeld's textbook and that of the course for which it was written. In the fall of 1946, Fred Keller and Nat Schoenfeld—in some respects the Lenin and Trotsky of our movement to overturn the accepted order in psychology—introduced a new curriculum for Columbia University's division for male undergraduates, known as Columbia College. (For additional background, see Keller, 1982.) The introductory course, which the authors subsequently described in considerable detail in an article in the *American Psychologist* (Keller & Schoenfeld, 1949), required two semesters to complete. From the very beginning of their contact with the discipline, students were required to take four hours of laboratory each week, as well as the more conventional two hours of lecture. Each pair of laboratory partners received an untrained rat. Using appropriately timed pellets of food as their only method of instruction, they taught their rat to press a lever inserted into its living-experimental cage and then studied its behavior throughout a series of 15 or so different experiments. The fundamental behavioral processes discussed in the lectures and directly observed during the laboratory sessions were to serve as the foundation for the more specialized courses to follow.[1]

During the first year under the new curriculum (1946–47), the teaching staff continued with a relatively brief resumé of behavioral principles that Keller had drafted for the earlier, lecture-only version of the course. But it was clear that this skeletal outline needed considerable fleshing out. As early as October of 1945, Keller had written to Skinner about his plan for an introductory textbook—"a Skinner for Beginners" (Skinner, 1979, p. 306). Some progress had been made the previous summer, but with the course under way, the need for a text became more pressing. The course was a considerable departure from anything that had been done before, and none of the textbooks then in existence covered even a fraction of the necessary material. Once the apparatus had been constructed and a sequence of experiments had been worked out, complete with procedure sheets and questions for the students to answer, the textbook became a top priority. By the beginning of the second year (fall of 1947), the authors were able to begin handing out a series of mimeographed chapters (King's Crown Press) that covered much of the final content. It was not until 1950, however, that the hard type, hardback edition was finally published.

I was a graduate student at the time, and my connection with the College was somewhat tenuous. Unlike the other participants in this symposium, I never served as an assistant in the laboratory version of the course. I *had* served as a

For their comments on an earlier draft of the manuscript, I thank Donald A. Cook, Daniel S. Dinsmoor—who once smuggled a baby rat from Columbia's vivarium to City and Country's "show and tell"—and Mary Janssen. Requests for reprints should be addressed to the author at the Department of Psychology, Indiana University, Bloomington, IN 47401-1301.

[1] Historians of psychology may be interested to know that an introductory course virtually identical to that at Columbia College was launched that same year at a college for female undergraduates. Tom Reese and John Volkmann had originally planned to collaborate with Keller at Columbia but had accepted new positions that fall at Mount Holyoke (see Keller, 1977, pp. 75–76). Volkmann, along with F. L. Blendinger and F. C. Frick, played a major role in the design and construction of the apparatus for both courses.

"reader" more than a year before when it was still a lecture course. As my main duties were to prepare and grade the exams, I became quite familiar with Keller's original outline, and the study of basic behavioral processes, as revealed in the animal laboratory, seemed to me to be the path to the future. As the time approached to begin work on a dissertation, I explored several possibilities in the area, finally settling on one suggested by Nat Schoenfeld.[2]

After a year away from the campus, teaching at what was then known as the University of Newark (1945–46), I had managed to secure a position in the School of General Studies at Columbia, where it was but a short distance from the classroom to the lab. It took five years for the authorities to pry me loose. For the first year, my duties were restricted to an introductory course in which standard textbooks were prescribed, but by the second I was also teaching a course in experimental method. A small room was set aside containing apparatus like that used in the College course, and I had the opportunity to try out my own sequence of experiments. (Some of the better known alumni of this course include Doug Anger, Alex Buchwald, Aubrey Escoffery, George Kish, and Bob Thompson.) I used the mimeographed version of K&S as my text. When the final version was ready for publication (Keller & Schoenfeld, 1950), I was invited to compile the index, an invitation which I eagerly accepted. Murray Sidman did the references. It was a privilege to be allowed to participate in so important an enterprise. Believe me, the perspective and goals of that book have had a formative influence on my thinking which lasts to this day. If there is one professional ambition that remains unfulfilled, it would be to write an up-to-date version of that book, incorporating the most relevant research that has appeared during the 40 years since its publication.

TWO FUNCTIONS

Keller and Schoenfeld's *Principles of Psychology* functioned effectively at two different levels: At the first level, it began the task of educating the public in behavioral principles and provided a medium for luring unwary undergraduates bent on careers in literature, mathematics, biology, or engineering into behavioral research. At the second level, it played an essential role in training one of the first crops of graduate students devoted to the experimental analysis of behavior. The course also helped: The same apparatus was used for dissertation research, and the assistantships needed to staff multiple sections of the laboratory provided considerable economic support. Although some well-known people had been trained at Minnesota (e.g., Breland, Estes) and at Indiana (e.g., Collier, Greenspoon, Guttman, Lundin, Wyckoff), the Columbia group was larger and exerted a greater influence on the subsequent development of the discipline.

I was never privy to the inner workings of the partnership, but I see both Fred and Nat as contributing very special strengths to the enterprise. One of the things that struck me about Fred (apart from his uncanny skill in reinforcing my behavior) was the interest he took in analyzing everyday behavioral episodes in terms of the principles of the conditioning laboratory—episodes which in turn could be used to illustrate those principles. The other was the skillfulness of his verbal behavior, both spoken and written. In my opinion, Fred Keller was and is a consummate carpenter of the English language, a shaper of the well-turned phrase, a master joiner of words. Do not let the simplicity of his writing deceive you. I remember the time when Fred had a run-in with the obituaries editor of *The American Psychologist*. The editor could not understand why Fred was upset at being asked to change the style in which his piece had been written. After all, it did not look as if it could have required

[2] Although I had difficulty coming up with a satisfactory idea on my own, perhaps I can at least take credit for choosing well: The resulting product was considered sufficiently significant to be reprinted some years later in a volume published by Gregory Kimble (Dinsmoor, 1967).

much effort. My suspicion was entirely different: I did not see how anyone could have expressed himself so concisely, so precisely, so aptly, and with such a rhythm, without having spent a good deal of time choosing his words. A labor of love, I would guess, and fortunately another outlet was found where it could be published in unmutilated form (Keller, 1981).

But clear and simple writing is not only an esthetic triumph, a joy to read. It can also serve a practical function. It can strip away the darkness surrounding a new set of ideas, revealing that which might otherwise remain obscured by the shadow. During the period when Keller and Schoenfeld were working on their text, many of us were reading a pair of books written by Rudolph Flesch, first *The Art of Plain Talk* (1946), and later *The Art of Readable Writing* (1949). Flesch had devised a procedure for quantifying the reading difficulty of samples of English prose and had shown a certain correspondence between the scores obtained with his method and more subjective reactions to the same material. Recently I applied Flesch's 1949 formula to a number of passages from *Principles of Psychology*. The average length of sentence in the samples I selected was 22.5 words, and the average number of syllables per word was 1.52. The overall score was 56, which places the material at a level of difficulty suitable for sophomores, juniors, and seniors in high school (Flesch, 1949, p. 149). That's pretty good for a text presenting findings from the conditioning laboratory as the foundation for a systematic science of behavior! For students who have gone on to college, it should be fairly easy. Not only does holding the difficulty down to a modest level make it possible for the less proficient readers to follow what is being said; it also makes the task less demanding—and therefore more inviting—to a group we would especially like to attract, those who perform at a higher level academically.

One of the things that K&S did, then, was simply to make Skinner's message *accessible* to a lay audience. It is difficult for a text that maintains its scientific integrity to compete with those that take the students' interest as their only guide. But another way in which Keller and Schoenfeld reached out to their readers was by showing them how the principles under consideration found expression in the details of their daily existence. By this time, Skinner had moved from Minnesota to Indiana and was beginning to do the same thing: "I had concluded *The Behavior of Organisms* by saying 'Let him extrapolate who will.' Indiana was the point of my extrapolation" (Skinner, 1988). But *Walden Two* (Skinner, 1948) was restricted in scope, and it was not until 1953 that the broader coverage in *Science and Human Behavior* made its apearance in print. That book went far beyond Keller and Schoenfeld in interpreting broad areas of personality and social psychology in terms of a limited set of basic principles, and much of Skinner's later writing reflects the same emphasis. Today, the younger generation, both basic and applied, seems curiously reluctant to interpret complex behavior in terms of laboratory principles, at least in print. Perhaps it is considered unsophisticated. But such extrapolations were an important factor in recruiting many of the pioneering figures in the experimental analysis of behavior. Teachers seeking to demonstate the relevance of behavioral principles to the world around us might be well advised to consult both Skinner's and Keller and Schoenfeld's introductions to the science of psychology.

A few years later, when I conducted a survey of my introductory class at Indiana after using K&S in conjunction with a more conventional text (required to cover certain assigned topics), I found that the students greatly preferred the more conventional text. I would not want to mislead anyone in this regard. However important it may be scientifically, research based on nonhuman subjects is greeted with a certain degree of antipathy by many members of the general public. Also, it takes much greater effort to understand material involving concepts that are not familiar and must be learned before completing the analysis. In terms of an economic model, an increase in the

work required per academic credit is disadvantageous to the student, and students frequently react negatively in their ratings (e.g., McKenzie & Tullock, 1978, pp. 290–298). (Never mind that little of intellectual value may be acquired when few new concepts are introduced.)

At Columbia, however, Keller and Schoenfeld's presentation proved extremely popular. Perhaps that was because students at Columbia College were a more select group. Perhaps the fulfillment of science requirements helped the cause. Perhaps the teachers were charismatic. Perhaps the opportunity to mold the behavior of a living creature in the laboratory sessions played an important role. The course was initially designed to accommodate 60 undergraduates; by the fall of 1948, 300 were applying each year, or one out of every five or six members of the eligible population (Keller & Schoenfeld, 1949). Furthermore, as I recall, at the graduate level most of the students at Columbia during the late forties and early fifties were either "rat men" (or very rarely, women) or "eye men" (students of Clarence Graham, who was also a solid behaviorist—see Graham, 1950). The surge of popular interest came just in time. In the second volume of his autobiography, Skinner (1979) noted that only 80 copies of *The Behavior of Organisms* had been sold during the war years, for a total of 550 copies from the time of its publication, and that the book was in danger of going out of print. K&S saved the day.

CONTRIBUTIONS

In a review of *The Behavior of Organisms* published last year (Dinsmoor, 1988), I noted that the basic ideas presented in Keller's original outline of behavioral principles—used before the text was drafted—could all be traced to Skinner's book. This might seem to imply that little had been added. But that outline would never have been written had Keller not recognized something which, despite Skinner's claims, was probably not that apparent to the casual reader. I hope that Skinner will forgive me when I say that to the uninitiated the B of O was a forbidding tome. It bristled with technical language, and most of its pages were devoted to textual or graphic depictions of how many times a white rat had depressed a lever in an otherwise almost barren experimental chamber. Even as a presumably bright graduate student, my first impression was that although Skinner's research was viewed with great respect by authorities in the field, the book was a recondite treatise that bore little relation to the kind of psychology in which I was interested, let alone to everyday affairs. If one *did* go on to study the book, one would find that in the early pages and again in the final chapter Skinner had expressed a belief that his research provided a foundation for a general science of behavior. But this was stated in abstract terms, and it was not obvious how or whether the task could be accomplished. Perhaps the conclusions *did* apply to behavior other than pressing a bar, but no examples were given. It took a Fred Keller and a Nat Schoenfeld to recognize the promise inherent in Skinner's concepts and to make that promise clear to other readers.

Keller and Schoenfeld's commitment to the construction of a systematic account of their subject matter made an explicit appearance here and there within the pages of their book, but it was nowhere more evident than in the article in which they described the new curriculum at Columbia College (Keller & Schoenfeld, 1949). In the first page and a half of that article, the strength of their verbal behavior was attested by the frequency with which they referred to the issue. They repeatedly deplored things like the "encapsulation of topics," the "fragmentation of knowledge," and "data unrelated by known and stated principles," while writing in an approving vein of a "coordinated viewpoint," "integration of the topics," and the "basic principles underlying all behavior." (See also Keller, 1973, pp. 141–142.)

Their task required a judicious selection. In essence, *The Behavior of Organisms* was a compilation of research findings, 457 pages in length. Obviously, some

of the findings had broader implications than others. Even to write an outline, let alone a book, it was necessary to determine which of those findings were of central concern to a general science of behavior and suitable for presentation to a nonprofessional audience. One of the major contributions of K&S, then, was to extract the important principles from the remainder of Skinner's work, bringing the system into sharper focus.

INTEGRATING THE DATA

Earlier in this paper, I suggested that both Fred and Nat had contributed some very special skills to their joint enterprise. What struck me about Nat when I was a graduate student was his skepticism, his unwillingness to swallow the free-floating, undefined verbiage that so often passed (and still passes today) as psychological theory. If Fred was Mr. Reinforcement, Nat was Mr. Discrimination. I doubt that I have ever met a more penetrating critic than Nat Schoenfeld. Some students feared his probes of their verbal behavior, but I had profound empathy for his questioning attitude and profound admiration for his questioning aptitude. In later years, I've done my best to emulate Nat in this regard.

Nat's ability to separate the observations from the verbalizations must have been especially useful, I suspect, when analyzing findings described in other theoretical languages. "Our systematic position has not kept us from looking at facts in other theoretical contexts. Good data are good data, regardless of theory" (Keller & Schoenfeld, 1950, p. 14). The chief reservation that reviewers had expressed when evaluating *The Behavior of Organisms* had been that Skinner did not discuss other people's findings. According to Wolf, for example, "the book as a whole is undoubtedly too exclusively personal. . . . Experiments with the albino rat are certainly not enough to establish a system of behavior" (1939, p. 478). Finan complained that "Unfortunately one finds that problems and solutions are evaluated by the author as unique findings. No serious attempt is made to relate the results to the accumulated literature on learning and conditioning" (1940, p. 445). Krechevsky, too, was enthusiastic about Skinner's empirical work but criticized him for failing "to tie up his work and his thinking with the rest of psychology's experimental data and concepts" (1939, p. 404). Hilgard felt that "it is unfortunate that Skinner did not do his readers the service of relating his system in greater detail to the experimental data of other investigators. . . . The difficulties in making extensions of the system may result in the book's being less useful, and perhaps less influential, than it ought to be" (1939, p. 124). It may have been unreasonable for the reviewers to have demanded, in effect, that Skinner write two books rather than one, when the one not only represented a lengthy program of research but also was destined to become one of psychology's most eminent classics. Obviously, Skinner considered it more fruitful to spend the time collecting his own data, rather than reading the works of others. Who is to quarrel with that? But the reviews did point to a *need*, in terms of public acceptance, that had not as yet been met.

At the most elementary level, K&S served the budding professional as a *compendium* of the literature on conditioning. To be sure, a substantial part of that literature was reviewed in books like Clark L. Hull's *Principles of Behavior* (1943) and Ernest Hilgard and Donald Marquis' *Conditioning and Learning* (1940). Hilgard and Marquis, for example, had a bibliography of 973 items. It was a useful volume. But their viewpoint, and consequently their emphasis, was different. Although their index listed 32 pages on which Skinner was cited, Hilgard and Marquis did not summarize anywhere near as much of his data as did K&S nor did they provide more than a brief sketch of his overall point of view. By contrast, Skinner's work served both as the empirical core and as the organizing framework for Fred and Nat's account.

But Keller and Schoenfeld's book was much more than a compendium. They not only presented work from other lab-

oratories, including a substantial amount conducted under their own auspices; they also *integrated* a variety of findings within a single theoretical structure. As Judson Brown put it in his review, "one of the important positive contributions of the text lies in its having achieved at least a partial coalition of Skinner's data and terms with the data of other behavior scientists" (1952, p. 192). I find this very important. For a system to be persuasive, it is not enough that the principles subsume the data from which they were derived. If the system is to be accepted as a general framework for a large portion of psychology, it is certainly encouraging and perhaps necessary to have demonstrations of what it can accomplish with data obtained with other response topographies, under other circumstances, from other species, and using other measures. Keller and Schoenfeld relied mainly on the rate of lever pressing as their preferred datum, but they also included results from other types of apparatus, such as the runway, the maze, Thorndike's problem box, the shuttle box, the activity wheel, the jumping stand, the obstruction box, and the Yerkes-Watson discrimination apparatus.

They also dealt with more complex cases in which the variables studied one at a time in the basic experiments were allowed to interact. At one level or another, the authors discussed such diverse topics as experimental neuroses, insight, the psychophysical methods, reaction times, word associations, code learning, depth perception, concept formation, transposition, the learning and forgetting of nonsense syllables, conflict, proprioception, thinking, emotion, dominance, cooperation, and imitation. The demonstration that other data, not gathered within the original instrumental or theoretical context, were amenable to interpretation in terms of the same concepts played a major role in convincing me, at least, that the system worked, that it *was* capable of integrating data from a wide variety of sources.

In addition, K&S had positive features that went beyond its role as an integrator of data. The title was taken from the classic by Herbert Spencer (1870), which anticipated Thorndike's Law of Effect. The authors were very aware of the history of psychology and placed their own contributions within that context. They took their readers seriously: At the end of each chapter, they appended notes directing the inquisitive student to further readings on selected topics, often from other theoretical perspectives. For some students, the book even served a therapeutic function, helping them to think more realistically and more precisely about their own behavior.

One of the most effective sections, in my opinion, was that dealing with verbal behavior. Remember that Skinner's own, more detailed presentation of his views did not appear in print until 1957. In the meantime, aside from mimeographed lecture notes, the material that appeared in K&S was the only available source for one of Skinner's most perceptive contributions. As a synopsis of "how verbal behavior can be subsumed within objective psychological science" (p. 378), Keller & Schoenfeld's account still has few to rival it.

As I said earlier, the book cannot wholly be divorced from the course. In the words of Donald Cook, who took the course as an undergraduate:

> The experience had powerful effects. The graphs you saw in the book were duplicated before your eyes. You could reconstruct the key assertions upon which the system rested. You developed an "animal faith" that the variables could be tracked down and put to work in shaping the actions of a single organism. Deep habits of experimentation and inquiry derived from that faith. Graduates of this system think alike, almost recognize each other within a few moments of conversation about the analysis of a behavioral process, and take a distinctively similar view of such issues—stressing always the role of consequences of behavior, usually some skepticism about merely telling people things, and considerable skepticism about punishment procedures. This degree of intellectual community fosters the capacity to work together as members of a common culture. (Personal communication dated June 20, 1989.)

That the program as a whole was effective in training experimental psychologists is attested by the fact that in addition to Fred and Nat, Columbia contributed five former students to

JEAB's first editorial board. All in all, the Columbia contingent accounted for 7 of its 16 members. Three of the first four editors-in-chief received their initial training in the experimental analysis of behavior at Columbia. Even today, when our ranks are much more numerous, Columbia graduates continue to be an important factor. It is difficult to obtain an exact count, but when I went through the list of Fellows for Division 25 in a recent APA Membership Register, I recognized at least one in every eight as Columbia alumni. There are probably others trained by the same system on other campuses as well as "grandchildren" trained by Columbia alumni.

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